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IMITATION IN MONKEYS.[1]

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The literature dealing with natural history abounds in anecdotes concerning the imitative power of monkeys. Thorndike, in his experimental study of the mental life of monkeys, denies that imitation plays an important rôle in the life of the primates. Students of behavior were inclined to accept the conclusions of Thorndike until Hobhouse's experiments were reported. As is well known, the latter finds that many animals, especially the monkey, learn both by 'perception of result' and by imitation.

Believing that a thorough reworking of the field was desirable, I began experiments upon four monkeys in the summer of 1906. From that time on to the present, the behavior of these monkeys has been observed incidentally under environmental conditions more or less resembling those offered by their native habitat, as well as under conditions of experimental control. In taking up the study, several essential factors were kept in mind. In the first place, the animals were purchased shortly after importation. They were kept, under the best conditions of housing, until they had become used to handling and until a complete knowledge of their repertoire of 'chance associations' had been obtained. Hobhouse experimented upon garden monkeys whose opportunities for the formation of coördinations of the most diverse kind had been legion. This fact, coupled with his rather loose type of experimentation, is enough to make one accept his somewhat eulogistic conclusions with a grain of caution.

[1] This MS. was prepared essentially in the present form for presentation before the Western Branch of the American Psychological Association in October, 1906. For a statement of recent literature upon imitation see review section of this number of BULLETIN, edited by the present writer.

In the second place, the animals were kept under normal conditions of hunger. During the period of experimentation, they were fed the usual amount of food. The stimulus used was always some form of food for which the monkeys had a special fondness, such as malaga grapes or bananas. These animals, after having been fed to satiety both with bananas and with bread and milk, will eagerly attack a problem-box if it contains grapes.

The four monkeys were selected from a group of nine on account of their gentleness or alertness. Of these, one was a baboon (*Cynocephalus*),[1] one a capuchin (*Cebus*), while the other two were rhesus (*Macacus*). The first two animals need no commentary. They were healthy and characteristic of their respective species. The two rhesus monkeys have a rather interesting history. Jimmie, a large male, was obtained from an importer; Billy, a small male, was a stray, probably from some circus. He and his mother (?) were found one day in the yard of a large apartment house. At the time, Billy was nursing and was carried everywhere by his mother in the characteristic maternal monkey fashion. On account of the fierceness of the mother, Billy was weaned and put into a large cage which housed Jimmie. An attachment sprang up almost immediately between these two monkeys. At this period of his life, Billy was very wild and restless. When excited, he would cry and attempt to attach himself to Jimmie as he had customarily attached himself to his mother. This Jimmie soon discouraged by severe nips and cuffs. A little later, after the friendship had been established, Jimmie, whenever Billy became excited, would run to him, assume a sitting posture and put his forelegs around Billy's neck. Billy, on his part, would nestle up to Jimmie and clasp him around the chest. If Billy did not become placid under this treatment, Jimmie would gently rock him from side to side and at times would pat him, giving out a soft companionable chatter. This friendship has continued up to the present time.

On account of the peculiar relationship existing between these two animals, a close watch of their daily life was kept. It soon became evident that Jimmie's reactions influenced Billy's to an enormous extent. When Jimmie goes to one part of the cage, Billy follows. If a pan of water or a bowl of milk be held out to them, Billy will come down to drink if Jimmie will precede him, never otherwise. Jimmie has formed the habit of jumping to my shoulder when I enter the cage and call; Billy has formed the same habit, but if Jimmy for any rea-

[1] This animal was so stupid in all his reactions that results obtained from him will not be considered specifically.

son refuses to come when I call, Billy will refuse also. If Billy is loose in the room when I pass with Jimmie on my shoulder, he will run to me immediately when I call, but on the other hand, if Jimmie is not with me, Billy will pay absolutely no attention to my commands. At times, Jimmie and Billy are left loose in the animal room. When I desire to force them to enter their cage, I get a long stick and threaten Jimmie with it. He usually stalks around the cage two or three times before entering it, but always just out of reach of the stick. Billy invariably 'tags' him and when Jimmie finally darts through the cage door, Billy plunges in too. It is almost impossible to force Billy to enter unless Jimmie has preceded him. On the other hand, if Jimmie is left in the cage and Billy is forced out into the room, the latter is unhappy and will reënter the cage the moment the door is opened wide enough to admit him.[1] Both monkeys are restless and excitable when they are in different rooms. At such times calls are frequently made to each other. When again united, they cuddle together in the manner described above, and both chatter in a way which is hard to describe, but which seems to be an expression of emotional satisfaction.

I have entered into a description of the companionship between these two animals, because under such conditions surely, if anywhere, we ought to be able to demonstrate intelligent imitation, provided such a function be possessed by them.

Problems of the manipulation type, and of a type involving for their solution, apparently, the presumable perception of a simple relation, were presented to all of these animals. These latter problems are designed after those employed by Hobhouse. Indeed, in some instances, I have tried to duplicate Hobhouse's conditions.

The following is a partial list of problems presented to each of the four monkeys:

A. Problems depending for their solution upon the perception of relation:

I. Drawing in food with a rake — animal to imitate me.

II. Drawing in food with a cloth — animal to imitate me.

III. Obtaining food from bottom of bottle by use of fork — animal to imitate me.

IV. Pushing out food from middle of long glass cylinder by means of light sticks — animal to imitate me.

B. Problems of manipulation type:

[1] As Billy increased in age many of his babyish habits tended to disappear. Recently he has manifested a certain independence in coming to the door of the cage for his food.

V. Old-fashioned latch — animal to imitate *Billy* or me.

VI. Box with door in top — not held in place by any fastening. Animal to pull open door by means of handle — Billy to imitate Jimmie.

VII. Box with door in top held in place by push-button. Push-button was attached to door and ran through guide in frame of door. Door opened (pulled up) when latch had been slipped back far enough to clear guide — Billy to imitate Jimmie.

Without entering into details concerning the learning process involved in these problems of the manipulation type, I unhesitatingly affirm that there was never the slightest evidence of inferential imitation manifested in the actions of any of these animals. There was never imitation either of my movements or of the movements of the animal which was successfully manipulating the mechanism. In problems VI. and VII. Jimmie almost immediately hit upon the successful movements. For three weeks he procured all of his own food in this way. Billy, who had previously easily learned box V., was present 'watching' all of his movements, but could never at the conclusion of the tests with Jimmie manipulate these boxes alone at his imitation-test (once each day for five minutes or longer, immediately at conclusion of Jimmie's exposition).

Jimmie is the quickest animal in learning mechanisms I have ever observed. When a box worked by any variety of mechanism is placed before him, he tests the various movable parts with teeth and paws with lightning-like rapidity. Nevertheless, after watching his young companion manipulate box V. for three days, he showed not the slightest familiarity with the mode of procedure required to open it. He learned it perfectly of his own accord, however, by a hit or miss method, in five trials.

Most of the tests of the manipulation type made with the cebus, Harry, were on box V. At intervals, for over a month, I presented this box to him: I would attract his attention to the food in it when he was within a few inches of the door; and while he was apparently attending to my movements, I would slowly raise the latch and allow the door to fly open. Over 100 trials of this kind were given him during the course of the experiment. During all this time, he failed to profit by my tuition. He was always given his imitation-test immediately after I had opened the box.

Another cebus, lately purchased by me, failed after 60 trials to open a small metal box which is opened by means of a small handle (similarly to VI.). A grape was placed in this box and the lid was

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then opened and shut before him several times. Invariably, when he tried to open the box alone, he clawed and bit at the edges of the junction point between lid and box until by chance his movements were successful. Not for the first 60 trials did he use the handle to pull the lid open.[1]

The results from the first four problems, which involve apparently the perception of relation, gave no better indications of the presence of imitation.

Problem I. was presented to Jimmie, Billy and Harry (the cebus) at intervals, for more than three months. In detail, the method was as follows: The animal was tethered in an open floor space. A grape was placed out of reach. A light, ten-inch stick with a two-inch T-piece fastened to one end was left near. In order to get the grape, the monkey would have to hook the T-piece behind the grape and pull in. As is known, Hobhouse found that his monkeys (rhesus and chimpanzee) would use a rake, a crooked stick, and even a piece of cloth for this purpose. I have been wholly unable to verify his results.

When the animal is placed in this situation, it at once begins to strain at the tether and to reach out with the paw as far as possible. All three of the monkeys were given several hundred trials without receiving aid from me. Never once was the slightest effort made to utilize the rake in the proper way. They would often pick up the rake, bite it and then discard it. I oftentimes left them in this situation for hours at a time. The food had never been reached upon my return.

Finally, I began showing the monkeys how to draw in the food. I would wait patiently until I apparently had their attention, then slowly hook the T-piece around the grape and slowly draw in the food. The moment the grape rolled near, the animal began to strain

[1] This box was to be used later as the food box in a discrimination test on spectral light. I thought the test on discrimination would be facilitated if the monkey were allowed to open the box in the daylight. After failing to teach him as above described, I gave up in despair and decided to try him in the discrimination test in the dark room, trusting that the conditions there, offering less distraction, might after all hasten the association. Strange to say, he manipulated the box perfectly from the first trial. The reason was apparent. In the dark room, a faint red light was presented with the box. Under these conditions, the animal began 'peering' about, with his head close to the floor, and accidentally struck the handle with his mouth, and immediately opened the box. Ever afterwards, he adopted this method. Harry, the other cebus, also learned to open the box with his mouth. The rhesus monkeys, on the contrary, opened it with the paw without any apparent learning process.

at the tether and to attempt to grasp the grape with the paw. The situation was again immediately arranged as before and the rake left near.[1] *Never once did any one of the animals push out the rake, hook the blade around the grape, and then pull in,* nor did they in any other way ever show any signs of perceiving the relation which ought to exist between the two objects. After many tests, the rake became associated with food and the animals began eagerly to pick up the rake and to drag it about the moment I came near with the food, only to drop it, and begin straining to get the food with the paw the moment the grape was put down on the floor. No effort was made to set up the association by the trial and error method, since it was desired to test only the possible presence of a higher form of learning.

The above test was repeated *ad nauseam* and I finally came to the conclusion that Hobhouse must have experimented upon a far more intelligent group of animals than the above; or that he was testing animals which had already learned a variety of such tricks, and consequently the apparently new reactions which he obtained were really due to extremely slight modifications of associations already existing; or, finally, that he was inclined to read more into the monkeys' use of the stick than was really present (the monkey will *pull in* a rope or stick with great rapidity and dexterity). It would be idle speculation on my part to decide among these possibilities.[2]

Problems II., III. and IV. gave identical results. In problem II. the cloth was drawn in if it were anywhere within reach, regardless of whether it were in a position to catch the grape in passing or not. Jimmie was fond of taking the cloth, putting it over his head and then dancing round the room. None of the animals made the slightest effort to throw the cloth out over the grape and then draw in, although they were shown the trick by me many, many times.

[1] At the first test, the monkeys would draw in the stick if it were left near the grape; but they would pull in just as eagerly if the blade was not hooked around the grape as if it were. Indeed, they would strain to reach the stick and pull in even when no food was near. It is an inveterate habit with them to pull in any object which can be reached and manipulated.

[2] To show Jimmie's level of intelligence, it may be worth while to mention the fact that he soon learned that by turning round and facing the tether and using the hind legs instead of the fore legs, he could increase his reach by several inches. In the later trials he adopted this method exclusively, gaining great adroitness in the use of the grasping powers of his hind feet. He succeeded many times in getting the grape because the experimenter would forget Jimmie's 'method' for the moment and would place the grape too near. Billy, his companion, never caught this trick, although he was constantly near Jimmie during the trials.

In problem III. a piece of banana was placed in the bottom of a tall bottle. A stick, roughened and sharpened at the end, was stuck into the banana. The end of the stick protruded from the bottle three or four inches. The animals on their first trial immediately grasped the stick and jerked out the banana. The experiment was then repeated, but the stick was not plunged into the banana. The monkeys as before jerked out the stick, but threw it down immediately and plunged the forearm into the bottle and attempted to reach the food by the more natural method. Not succeeding, they knocked the bottle down and rolled it around until the food dropped out. Jimmie, after several trials, learned to pick up the bottle by the lower end and to shake the food out. I tried patiently to teach them the use of the stick but failed signally.

In problem IV. the animal had to push a piece of banana from the middle of a 15-inch glass cylinder of 1⅛-inch bore. The glass cylinder was securely fastened to a table top. A light 16-inch stick was placed near. The monkeys, on being admitted to the apparatus, immediately began clawing and biting at the cylinder at a point nearest the banana. Later in their first trial, they came to one end of the cylinder and began a frantic struggle to reach the food with the paw. Their own unaided efforts were all of this random type. Day after day I took the light stick, put it into the cylinder and then slowly forced the banana out: The sight of the moving banana made them eager — they followed it down the cylinder until it came within reach of the ubiquitous paw. Meeting no success with this procedure, I changed the method by putting the stick inside the cylinder and just in contact with the banana. The monkey, in order to reach the food, would simply have to force the stick on in. Invariably, after my oft-repeated lesson, the monkey on his imitation-test first jerked out the stick, then began his random instinctive efforts to extract the food by means of his paw.

One other test, involving also for its solution apparently the perception of a simple relation, was given to Jimmie and Billy. Early in the history of their friendship, Jimmie formed the habit of taking Billy's food away from him — going oftentimes even to the length of poking his paw into Billy's cheek pouches. I thought it possible to test whether Jimmie would in time learn to push Billy out for food. The method was as follows: Jimmie was tethered short to one corner of the large living cage; Billy was left untethered but had a two-foot chain attached to his collar.[1] Food was placed in a box in the corner furthest

[1] It needs to be mentioned in this connection that the two monkeys were ordinarily kept in the cages with their chains on but unattached. In their play, consequently, Jimmie had already learned to haul Billy about the cage by using the latter's chain.

from Jimmie. Billy usually remained near Jimmie until food was placed in the box. He would then dash to the box (provided the experimenter would withdraw) and pick up a bit of food. Jimmie would immediately grasp the end of Billy's chain, draw him in and rob him of the food. He became extremely adept in doing this and during a day's experiment (ten to twelve separate tests) Billy rarely kept possession of a morsel of food unless he swallowed it instantly Jimmie soon began to grasp and to hold the end of Billy's chain the moment I brought in food. The performance of this trick looked at first as though it called for the actual perception of relation on the part of the animal, but closer observation showed that Jimmie would pull Billy back before he obtained food as often as he would allow him to get food before pulling him in. Billy finally learned to circle — to leave from a position where Jimmie could not grasp his chain; after getting the food, he would climb upon a board near the top of the cage. In doing this his chain hung down, whereupon Jimmie would catch the chain immediately and pull Billy down. Billy on his part never learned to keep his chain out of Jimmie's reach.

For three weeks Jimmie was forced to get all of his food in this roundabout way. The next step in the problem was to test Jimmie's behavior when Billy had been surfeited and would no longer go to the food box. In order to arrange this, Billy was taken out and fed until food would no longer tempt him to move. He was then put back into the cage with Jimmie and food was placed in the food box as before. I desired to see whether there would be any effort on Jimmie's part, under these circumstances, to force Billy to go towards the food-box (pushing and pulling Billy about was one of Jimmie's pastimes). Under the conditions mentioned Jimmie would begin immediately to draw in Billy by his chain and to haul him back and forth, but this random activity was all — *there was no tendency present to push Billy toward the food.*[1]

Such are the experiments which closely engaged my attention for about a year (June, 1906 to April, 1907) and incidentally for more than two years. From all this observation, I am forced to conclude that imitation in its higher forms has not been a very powerful or efficient means of aiding the monkey in reaching its present high place in the mammalian series. The reason for the primate's superiority is to be sought for in his greater sensitiveness to an extremely wide range

[1] Nor did Billy ever gratuitously attempt to supply Jimmie with food. Garner states that when one of his pet monkeys became sick, its playmate and companion carried food to the invalid.

of stimuli and to his superb power of muscular response rather than to any ability on his part to construct stimuli into definite objects which can be analyzed and synthesized by him, later, at will.

The above represents the results obtained from my controlled experiments. In my long association with these and other monkeys, I have incidentally observed certain types of reaction which are circular in character and which are suggestive of a low order of imitation. I append examples of these below. The examples chosen are by no means exhaustive.

Evidence for Circular Types of Reaction.

Looking Through a Crack. — One monkey discovered a hole in a window-frame where a sash-cord had formerly run. This one would 'peek' and then another one would push him aside and 'peek' in turn. This was observed several times when several monkeys (rhesus) were brought up from the dealer for examination and selection. It was later observed in two monkeys which had been in the laboratory for some time (Kinnaman has observed the same type of reaction).

Dropping a Spoon. — While the monkeys were at liberty on Bird Key (Dry Tortugas), I was disturbed one day by a noise: I found that Harry (cebus) had filched a large tablespoon. He was standing the spoon on one end and immediately releasing it. The dropping seemed to be not accidental but an actual part of the act as a whole. (The cebus is extremely adept in the use of his paws.) He repeated the act fifteen times in unvarying order and (as well as I could judge) at definite time intervals. This corresponds, in my opinion, very closely to the child's act in repeatedly hammering its spoon against a dish.

Hammering with Nut (or any small, hard, preferably round object).— The cebus, Harry, will take a hickory nut to some hard surface and hammer with it at intervals for several moments. Short series of taps, averaging four to six separate taps to each series, take place in quick succession — 150 such series were once counted in one half hour. Another cebus, Sammy, exhibited the same reaction. In the forest, this act is probably connected with some kind of food-getting process. It has no significance in the present environment of the animals, but seems to have been retained for its value as a circular form of play. A spoon, piece of metal, small rock, etc., will often be taken to the window-pane by Sammy and used as a hammer in this way.

The rhesus, Jimmy, also gives evidence of such a type of reaction: Occasionally (but rarely) he will sit and play with a hard object and

let it slip idly through his fingers to the board upon which he sits, pick it up again, and again drop it. Putting a handkerchief or towel over his face, then removing it, and repeating the process over and over again is another form of Jimmie's play.

I have found it possible to instigate Jimmie to perform one rather interesting instinctive act: Flea-catching, regardless of what the sociologist may have to say, is the most fundamental and basal form of social intercourse between rhesus monkeys! The act is well known. As the monkey works over the body of his companion with his paws, he smacks his lips together continually and occasionally brings one paw to the mouth. This smacking sound is the invariable accompaniment of the act. It can be imitated easily and perfectly. I was able to get Jimmie, on one occasion, to come to me and to let me 'pick' him. After I had performed the act satisfactorily to him, he perched upon my shoulder but made no attempt to 'pick' me. I held up the hairy part of my arm to him, but he still made no effort in that direction. I then began to make the smacking sound with my lips. He immediately made the sound in turn and began searching my arm and then proceeded to my neck (I was wearing a rubber cap over my hair). On two or three other occasions, I was able to repeat this, but I cannot produce the act at will.

The examples cited, taken in connection with the behavior of Jimmie and Billy described in the early part of this paper, will serve to show that we have in the reactions of the monkeys, at least a rudimentary type of imitation.

The anecdotal material which I have collected would compare favorably with that presented by Romanes and others, but close examination of such acts, especially during the period of their genesis, does not lead me to think that the higher forms of imitation are present in them.[1]

[1] Dr. Karl T. Waugh has undertaken to repeat the experiments of Berry (see review section) upon these monkeys. The writer wishes it understood that his present conviction upon the subject of imitation stands ready to be changed as soon as the evidence calls for it.

PSYCHOLOGICAL LITERATURE.

PROGRESS IN THE STUDY OF THE BEHAVIOR OF THE LOWER ORGANISMS DURING THE PAST YEAR.

The most important advances in this field have been (1) in the study of the formation of habits, associations and other modifications of behavior, and (2) in researches on reactions to light. We shall take up the more important works under these two heads, then deal with some miscellaneous matters.

1. Formation of Habits, and Other Modifications of Behavior.

(*a*) *Protozoa.* — The behavior of the Protozoa continues to become more and more complex with the advance of precise knowledge. Metalnikow[1] describes experiments which seem to show that the infusorian *Paramecium* sorts over and selects its food, and that this selection becomes modified by the experience of the organism. The prevailing belief that the infusoria make no selection in food is based largely on the fact that inert substances, such as carmine or India ink, are taken as readily as useful food particles. But according to Metalnikow, if the animals are left for several days in water containing such inert particles, they cease to take them, though *they continue to take other particles of food* that are mixed with these inert particles. This would require a definite power of sorting over the food particles, rejecting some and admitting others, together with capacity for the educability of these powers — properties claimed for infusoria by Hodge and Aikins as long ago as 1895, but not commonly admitted.

It should perhaps be said that certain attempts to confirm the results of Metalnikow, under the direction of the present reviewer, have not as yet been successful. But unquestionably a thorough study of the food reactions of the infusoria would give results of more importance for judging the real nature of their behavior than perhaps any other line of work.

Stevenson Smith, in a paper read before the International Congress

[1] Metalnikow, S., 'Ueber die Ernährung der Infusorien und deren Fähigkeit ihre Nahrung zu wählen,' *Travaux de la Soc. Imp. d. Naturalistes de St. Petersbourg*, 1907, 38, 181–187.

of Zoölogy in Boston, described experiments which seemed to show clearly that *Paramecium* modifies its behavior with experience. The animal was placed in a capillary tube so narrow that it could turn around within the tube only by doubling on itself. Coming to an obstacle (the surface film at the end of the water column), it endeavored to avoid this in the usual 'trial and error' way — backing, and turning repeatedly in different directions. Finally it doubled over its body, and returned on its course, thus escaping. On repetition of this experience it gradually lost its tendency to react on the 'trial and error' plan; on reaching the film it at once doubled and turned directly, retracing its course. The work of Smith seems accurate and clear. A full account is to appear in the *Journal of Comparative Neurology and Psychology*.

(*b*) *The Lower Invertebrates.* — Perhaps the most important and interesting series of investigations in recent times on behavior of lower organisms is that of Georges Bohn and associates, dealing with the part played by the past history of the organism in determining its present reactions. These investigations deal mainly with the life of the animals of the seashore. The separate observations by Bohn, Piéron and others have been recorded as they were made, in numberless brief notes mainly in the *Comptes rendus de la Société de Biologie*. From time to time the results have been brought together in more unified form in publications of the Institut Général Psychologique. A recent contribution by Bohn[1] gives a useful and interesting general account of this work, with particular reference to the sea anemones and their relatives. I shall try to bring out some of the main facts.

At the seashore we find such a complex of continually changing conditions as to constitute, as Bohn remarks, 'a vast field of experimentation.' For example, all the numerous animals of the seashore require certain amounts of moisture; some more, some less; some live in the dry sand, others beneath the water, and there is every gradation between these extremes. Now, there are repeated changes in the amount of moisture of any given region of the shore, owing mainly to the periodic alternation of the tides, secondarily to the alternations of day and night, and in minor degree to other causes. The animals are forced to adapt themselves in some way to these changes, and this results in producing periodic changes in their activity. For a certain time the snails of the rocks, being moist, come out of the crevices,

[1] Bohn, G., 'Introduction à la psychologie des Animaux à symétrie rayonnée,' *Bul. Institut Général Psychologique*, Nos. 1–2, 3–4, 7e année, 1907 87 pp.

creep about and carry on their other life activities; then for a period, being dried, they retreat into crevices, close their shells, and remain inactive. The mudworms remain at the surface for a time, then creep into their holes. The sea anemones extend for a time; then as the tide falls, they collapse, and so remain till the tide again rises. We have then a grand experiment in progress, on the effects of periodic changes in external conditions on the behavior of animals.

The effects of these periodic changes are found not to pass away at once; they leave certain effects on the animal which show themselves in the future behavior, even when the organisms are removed to uniform conditions, as in the aquarium. We find here illustrated on a grand scale and in a most striking way the fact that the reactions of even lower organisms are not definite, fixed, final; but that they depend on the past history of the individual. At the seashore, says Bohn, "one can thus study with ease the way in which the diverse factors of the environment fashion, as it were, the living material."

The effects of past history on present reactions are shown in numberless ways; we can list but a few typical cases from among the many described by Bohn.

Many specimens of the sea anemone, *Actinia equina*, live attached to stones in tide pools. When the tide is low they become partly uncovered; the water stagnates; there is too little oxygen, and the animals close tightly. Now, the first sign of this condition is a cessation of wave motion as the tide goes down. As soon as the waves cease, the *Actinias* at once collapse; they do not wait till the amount of oxygen has really decreased. When the waves begin again, the animals at once expand. It is possible to induce them to expand by making artificial waves, or to contract by preventing the waves from reaching them. But this reaction is variable, *depending on the tide period.* If the animals are placed in an aquarium where the water level does not change, at the period for high tide any slight shock causes them to expand and remain expanded for some time. But during the period for low tide such a slight shock has no effect; the animals are accustomed to remain contracted during this time, so that they can with difficulty be induced to open; if they do open at all, they quickly close again. At the time of low tide any of the various stimuli that tend to cause contraction, act with redoubled effect, while stimuli causing expansion act with redoubled effect at the time of high tide. Sometimes the effect of a slight shock is quite reversed in the two periods; at high tide it causes expansion; at low tide, contraction. The periodicity goes in some cases so far that the animals in the aquarium

spontaneously contract and remain contracted during the period of low tide, while at high tide they extend and remain open. *Actinias* that had lived in deep water, not subjected to tidal action, do not show this periodicity; it is an effect of the past history of the individual.

In many other ways the behavior of the sea anemone depends on its past history. Specimens which have lived in a well-lighted region contract less readily in response to light, and expand more readily in response to other stimuli, than do those from shaded regions. There is a tendency to establish a periodicity corresponding to day and night similar to that due to the tides. Different specimens (of the same species) show, under uniform conditions, different periodicities, depending on their earlier history and habitats. "The *Actinias* which live on the vertical walls of rocks at high levels and which thus undergo drying at low tide have the rhythm of the tides; those which live in pools of water at the same level, constantly under the water, present the rhythm of day and night, and this also persists in the aquarium. Often masked by present causes, it becomes very evident in a medium of such remarkable constancy as that of a dark chamber" (Bohn, *l. c.*, p. 31). Many experimental demonstrations of these periodicities are described by Bohn; the combination of the two natural rhythms, together with modifications due to laboratory treatment, often produces results of much complexity and apparent capriciousness — though strictly determined by the past history.

A peculiarly interesting effect of past conditions was shown by Bohn's associate, Van der Ghinst.[1] Some of the individuals of *Actinia* live (1) on the under side of rocks, with the disk directed downward; others (2) on the upper surface of rocks, with the disk directed upward. When placed in an aquarium between an upper and a lower plate of glass, each set takes the position it had in nature; the first set on the upper glass, with disk downward, the second set on the lower glass, with disk upward. If forced to take unaccustomed positions, they lose after 24 to 48 hours the tendency due to the original habitat.

Other animals of the seashore show similar phenomena. The snails of the genus *Litorina* react negatively to light at the time of high tide, when the waves rise and strike the rocks on which they are found; they thus creep into holes and are protected. When the waves fall and their shocks cease, *Litorina* becomes positive to light, comes out of its holes and creeps about. Bohn shows that even after the animals are brought into the laboratory, at the time of high tide slight

[1] Van der Ghinst, I., 'Quelques observations sur les Actinies,' *Bul. Inst. Gén. Psychol.*, 6, 268–275.

shocks cause them to become negative and withdraw; while at the time of low tide they remain positive in spite of shocks. If we place *Litorinas* between two screens, one light, the other dark, those individuals that have been living at high levels and have therefore often been dried in the light, go toward the dark screen; those that have lived at low levels and so have not been dried in the light, go toward the light screen. The annelids *Hedista* are negative to light at high tide (going into holes), positive at low tide (coming out of holes). If in the aquarium they are placed between two screens, one black, the other white, they tend for about the six hours of high tide to go toward the black screen; for the six hours of low tide, toward the white screen.

The facts brought forth by Bohn, showing the effects of past history on reactions to present conditions, are impressive from their great number and the minute details in which the influence shows itself; a full account of them should if possible be made accessible in English, as they throw a most important light on the nature of behavior. Bohn expresses, evidently with much ground, the following general conclusion: "These phenomena of association appear to me excessively important; since the work of the Americans this seems to lie at the basis of the psychology of higher animals; I believe that it ought to form equally the basis of the psychology of the lower animals; thus one is obliged to make these phenomena intervene constantly in order to explain the reactions of *Actinias* or *Litorinas*" (*l. c.*, p. 77).

Bearing on these same aspects of behavior are the papers of Glaser on the brittle star, and of Jennings on the starfish. Glaser[1] studied in the brittle star the 'righting' reaction and the removal of a rubber tube slipped over the base of an arm, with reference to possible improvement with experience. He found, as Preyer had done, that the animals solve these 'problems' in many different ways. But if presented the same 'problem' many times in succession, the animals did not improve, by reducing either the time taken or the number of useless movements. Glaser concludes that there is no indication of 'intelligent' action. He believes that the lack of modifiability is due to the 'versatility' of the animals; since they can solve the problem in any one of many ways, there is no strong reason why one should prevail over the others.

Jennings[2] reached with the starfish the same negative results when the animal was left to itself. But the experimentation was carried a

[1] Glaser, O. C., 'Movement and Problem Solving in *Ophiura brevispina*,' *Journ. Exp. Zoöl.*, 4, 1907, 203-220.

[2] Jennings, H. S., 'Behavior of the Starfish *Asterias forreri* de Loriol,' *Univ. of California Publications in Zoölogy*, 4 (no. 2), 1907, pp. 53-185.

step farther; in studying the righting reaction the experimenter actively intervened in such a manner as to prevent the starfish from succeeding in any way save one. When thus 'trained' the animal formed definite habits with some readiness. When the animal is left to itself, such habits gradually disintegrate under the action of other influences tending to cause variations in behavior; some were observed to last as long as a week.

The work of Jennings is an extensive general study of the behavior of the starfish, including a detailed study of the action of the pedicellariæ in protection and in capturing food; the food reactions in general; positive and negative reactions to the common environmental conditions, and a particularly thorough study, illustrated by numerous photographs, of the righting reaction. "Perhaps the most important thing developed in the paper is the demonstration of the variability, modifiability, unity and adaptiveness of the main features of the behavior of the starfish. The movements are shown to depend on the varying physiological conditions of the animal, and the numerous factors which demonstrably modify the physiological condition, and therefore the behavior, are set forth in detail. Habit formation is demonstrated and discussed in full. The unity and coördination of much of the behavior is presented and some theories of its nature and origin discussed. Of subordinate importance are the essentially new, correct accounts of the method of locomotion and of the way in which the negative reactions occur" (p. 183).

2. Reactions to Light.

One of the most important papers that has ever appeared in this field is that of Cole.[1] Leaving the traditional groove, the author presents problems and results that are new. He studied in a number of lower organisms the image-forming power of the eyes — what might be considered the beginning of the power to see objects. The animal was placed midway between two sources of light, of such strength and distance that (measured) light of equal intensity reached it from both sides. But one source of light was very small, while the other was large — so that the two would produce different images, in image-forming eyes. It was now found that some lower organisms react with reference to both lights equally, while others react mainly with reference to the light from the source of larger area. In the former group the reaction apparently depends only on the intensity of the

[1] Cole, L. J., 'An Experimental Study of the Image-forming Powers of Various Types of Eyes,' *Proc. Amer. Acad. of Arts and Sci.*, 42, 1907, 335–417.

light reaching the organism; to this belong the earthworm, the larva of the meal worm, and apparently certain others. In the latter group, where there is evidently the beginning of the power of forming images, of 'seeing objects,' were found the butterfly (*Vanessa*), the water scorpion (*Ranatra*), the cricket frog (*Acris*), the green frog (*Rana*). A number of others showed slight indications of the power to form images; among these are the flatworm (*Bipalium*) and the sowbug (*Oniscus*). Cole's work seems to answer definitely the old question why the positively phototropic moth does not fly toward the sun or moon; it is because areas of light on the earth are much larger, though not so intense.

This work of Cole's seems fundamental in character; it will probably serve as a point of departure for much future work, that shall give us a real understanding of the complex and varied ways in which lower organisms respond to the stimulus of light from objects. This work will have to take into consideration size, form, pattern, motion, color, and associative processes as possible determining factors.

Minkiewicz,[1] like Cole, has given us an important study in reactions to light that is quite out of the common groove; he justly introduces his paper with the statements that "In the question with which I deal I have no precursors." "All that will be found in what follows — the facts, the method of analysis, the way of looking at the facts in their ensemble — all is new."

Certain crabs are accustomed to disguise themselves by attaching small foreign objects over the surface of the body. The author studied this behavior in relation to colors. The crabs were placed in aquaria having, in different cases, linings of different colors, and were then supplied with an assortment of bits of paper of varied colors. The crabs thereupon selected out the pieces that corresponded to the background on which they were situated, and fastened these over the surface of the body! In a white aquarium they disguised themselves with white paper, in a green aquarium with green, in a red one with red, etc. Only in a black aquarium did this correspondence fail.

After the crabs have disguised themselves, they are placed in another aquarium, the two halves of which are lined with different colors. The crabs thereupon betake themselves to that half of the aquarium which corresponds in color to their disguise; those clothed in green go to the green half, those in red to the red half, etc. Experiments show

[1] Minkiewicz, R., 'Analyse expérimentale de l'instinct de déguisement chez les Brachyures oxyrhynques,' *Arch. de Zool. Expér. et Gén.*, 1907 (4), 7, notes et revue, pp. xxxvii–lxvii.

that this is due, not directly to the color of the coat worn by the crab, but to the fact that the crabs betake themselves to a region colored the same as that in which they have been before.

The author thus shows that the crabs react differently to different colors; they are negative to one color, while positive to another. To this demonstration of the effects of *color*, as contrasted merely with the effects of light, the author justly attaches great importance; he holds it to negative many of the current wide generalizations as to the uniform action of all the rays, the exclusive importance of direction of rays, of intensity, or the like.

In an attempt to express and explain the facts in a purely objective way, the author proposes numerous new technical terms, and gives an extensive general discussion of different ways of looking at such phenomena. However looked at, the unexpected demonstration of the extensive powers of 'color vision' possessed by a lower animal, and of the highly adaptive behavior in which this results, is of the greatest interest.

Walter[1] has made a detailed study of all the reactions to light in the flatworm — an organism showing none of the 'image-forming' power described by Cole. The experimental work is of the most thorough and satisfactory character; undue schematizing is avoided, and all sorts of factors are taken into account: the intensity and direction of the rays; effect of changes in intensity of the light, the complicating effect of other stimuli, effects of fatigue, varying physiological states, effects of habit, individual differences, etc.; further all the different methods of reaction are taken up in detail. Thus the work is one of the best for obtaining an idea of the complexity of the phenomena in the reactions of even a simple organism to a single kind of stimulation. But of course the conclusions to be drawn are much less sharp and superficially satisfactory than those that can be drawn from superficial work; for such facts as the following have to be considered: "Negative planarians frequently take an apparently positive course, because the impulse to move in any direction is greater than the phototactic impulse. The normal negative phototaxis of a worm may change temporarily to positive by reason of some physiological state which is not obviously referable to external stimuli" (p. 86). "Whatever the part played by light in their behavior it must always be an exceedingly varied and complex one." This of course makes the paper difficult to abstract satisfactorily.

[1] Walter, H. E., 'The Reactions of Planarians to Light,' *Journ. Exper. Zoöl.*, 1907, 5, 35–162.

As to the nature of the stimulus, the following conclusion is drawn: "The behavior of planarians may in general be more satisfactorily explained, by regarding, with Loeb, the intensity rather than the direction of light as the principal operative factor in light reactions" (p. 140).

Certain points in the author's general discussion, ostensibly directed against the present reviewer's views, call perhaps for a word. Replying to criticism of the tropism theory, he says: "The tropism theory, on the contrary, is based upon asymmetrical action as the result of asymmetrical stimulation." "It seems to me that the mechanism by means of which the asymmetrical response is brought about is immaterial, so long as the response can be shown to be the result of asymmetrical stimulation." Taking this as the criterion of the tropism, I am of course an ardent disciple of the view that most directive reactions (including those of man) are tropisms. No one, so far as I am aware, has been so mad as to deny that animals commonly respond unsymmetrically to unsymmetrical stimuli. Thus, in my *Behavior of the Lower Organisms* (1906), the following occurs: "In still other cases the reaction shows definite relation to the localization of the stimulus, yet it is not due to local reaction of the part stimulated, nor is it brought about by trial. . . . The flatworm turns toward or away from the side stimulated, by reactions involving the muscles of both sides. . . . Innumerable instances of this class of reactions could be given; *they include perhaps the greater number of the directed movements of organisms*" (p. 307). Walter has added nothing to the analysis of the unsymmetrical reaction of the flatworm, made by Pearl, and quoted *in extenso* in my book, and his results and views are in full accord, so far as I can see, with my interpretation of the matter (*l. c.*, p. 273). The only ground for discussion is the propriety of my use of the expression 'tropism theory.' My criticism was directed against a certain view of the mechanism of such reactions — a view which made this mechanism extremely simple. For calling this 'tropism theory' I am perhaps open to criticism, but it is important not to mistake a difference in usage of words for a difference of opinion as to the facts. To hold that "the tropic form of response may, and probably does, require a more complex mechanism than that which causes the motor reflex," that "it does not necessarily depend upon the direct stimulation of the motor organs, nor is it essentially stereotyped in character any more than are trial and error responses by motor reflex or random movements" (Walter, p. 153), is to hold the same views which I have urged, but to give them the name tropism

theory (a procedure which of course I have no desire to criticize). The above quotations, had they come from my own pen, would have differed only by beginning 'responses that have often been considered tropisms,' instead of 'the tropic form of response'; that is, I criticized the tropism theory as a certain narrow conception of the way reactions occur. Tropism is as good a name as any for 'asymmetrical reactions to asymmetrical stimuli' if we can agree that this is all we mean by it. Unfortunately, in my large collection of definitions of tropism, gathered from all sources, this one has not hitherto occurred. My endeavor to ward off a narrow conception of reactions in the lower organisms as simple and uniform has apparently been so successful as to leave nothing but the use of words to strive over. Essentially the view which I have tried to emphasize is neatly compressed by Walter into 'tabloid form' in the sentence "strictly speaking, all behavior is individual behavior" (p. 97).

Esterly[1] gives us a study of some of the peculiar interrelations of stimuli, in the effects of light and gravity on the daily migrations of certain crustaceans. In the light these animals are positively geotropic; during the day, therefore, they descend into the depths. In the dark they are negatively geotropic, coming to the surface at night. They are at all times negative to light, but the reaction to gravity overcomes the reaction to light.

Hadley[2] has studied the reaction to light in young lobsters, in various larval stages. An interesting point is the change in the character of the reactions with increased age. In the youngest stages the animals orient with anterior end *away* from the light, then swim backward *toward* the source of light; they gather in lighted regions. In older larvæ the orientation is less precise, and they gather in darkened regions. The effect of blinding one eye, in the younger stages, is to cause perfectly definite changes in the movements. The swimming appendages on the blinded side beat less strongly, those on the other side more strongly. As a result the animal turns or rotates over toward the blinded side. In consequence of this it may now, under certain conditions, in becoming oriented with anterior end away from the light, turn first directly toward the light; it then continues turning for more than 180 degrees, till its uninjured eye is directed away from the light. The results are due, the author holds, to a specially in-

[1] Esterly, C. O., 'The Reactions of *Cyclops* to Light and to Gravity,' *Amer. Journ. Physiol.*, 1907, 18, 47–57.

[2] Hadley, P. B., 'The Reaction of Blinded Lobsters to Light,' *Amer. Journ. Physiol.*, 1908, 21, 180–199.

timate connection between the eye and the swimming appendages of the same side. Whether the imperfection of the reaction can be corrected by experience, as Holmes showed to be the case in *Ranatra*, the author did not determine.

In view of the definitely determined character of the responses, the author contrasts them with the 'method of trial and error.' Without wishing to discuss this view for the case in hand, it needs to be emphasized that if the decision whether a reaction is by 'trial and error' depends on whether the movement is definitely determined or not, then the question is an idle one, and there is no such thing as 'trial and error.' There is no scientific ground for holding that undetermined movements exist in any organism whatsoever. The present reviewer used this designation for certain features of the behavior of lower organisms, because he found that they agree with certain behavior of higher animals that had received that name, in the fact that certain parts of the reaction do not tend to produce the result finally reached, and that the stimulation in consequence continues or is increased, so that the reaction is continued till it does produce that result. How the different features of the reaction are determined is a question to be worked out for each case. It appears worth while to distinguish this style of behavior from that in which all the movements tend to produce the result finally reached; and it will be well if someone can devise for it an appropriate name that cannot be misunderstood by those whose way of thought is incorrigibly anthropomorphic. The 'method' is really an example of that *greater permanence of certain of the combinations produced by varied activities*, which is so common everywhere, and which has often received the anthropomorphic name of *selection*.

3. Miscellaneous.

Holmes[1] studied the behavior of a large infusorian, *Loxophyllum*. While its reactions fit the general schema of infusorian activities, Holmes found its behavior considerably more varied than that of *Paramecium;* in this respect it evidently comes nearer to *Stentor*. The behavior of pieces was studied; the author finds that they react in essentially the same way as does the entire animal. Holmes is convinced that the behavior is a factor in regeneration.

Bancroft[2] demonstrated by adding India ink to the water that in

[1] Holmes, S. J., 'The Behavior of *Loxophyllum* and its Relation to Regeneration,' *Journ. Exp. Zoöl.*, 1907, 4, 399–418.

[2] Bancroft, F. W., 'The Mechanism of the Galvanic Orientation in *Volvox*,' *Journ. Exp. Zoöl.*, 1907, 4, 157–163.

Volvox the electric current causes a cessation or diminution of the stroke of the cilia at one pole of the organism; the creature therefore turns directly into orientation, as do most infusoria. Although the same method showed no such effect on the beat of the cilia in the case of the reaction to light, Bancroft is nevertheless inclined to assume that orientation to light is brought about in the same way.

Harper[1] contributes an interesting though somewhat abstractly written account of the reactions of the larva of a mosquito.

V. Uexküll[2] contributes an interesting account of the method and mechanism of locomotion and burrowing in one of the heart-shaped sea urchins, with particular reference to the nature of muscle and the part played by it and by the nervous system.

Piéron[3] attempts to show, both for protective quiet, or what is commonly called 'death feigning,' and for autotomy, or the cutting off by a crab, insect, etc., of one of its own limbs, that the action is not always purely reflex, but is at times rather of the same order as the so-called 'voluntary' or 'psychic' behavior. Drzewina[4] tries to show that Piéron's grounds for distinguishing a 'voluntary' autotomy are not well founded. Holmes[5] gives a general and somewhat popular account of the facts of 'death feigning.'

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RECENT RESEARCHES ON THE BEHAVIOR OF THE HIGHER INVERTEBRATES.

I. Audition.

Do the hymenoptera hear, has long been a debated question. Buttel-Reepen[1] insists that bees hear, because they respond, in a definite manner, to sounds made by their own kind. It has long been

[1] Harper, E. H., 'The Behavior of the Phantom Larva of *Corethra plumicornis* Fabricius,' *Journ. Comp. Neurol. and Psychol.*, 1907, 18, 435-455.

[2] Uexküll, J. v., 'Studien über den Tonus,' IV., Die Herzigel, *Zeitschr. f. Biol.*, 49, 307-332.

[3] Piéron, H., 'Contribution à l'étude de l'immobilité protectrice,' *C. R. Soc. de Biol.*, 1908, 64, 184-186; 211-213. *Id.*, 'Recherche sur l'autotomie: De l'existence d'une autotomie psychique superposée à l'autotomie réflexe,' *Arch. Internat. de Physiol.*, 1907, 5, 110-121.

[4] Drzewina, Anna, 'Sur la prétendue autotomie psychique,' *C. R. Soc. de Biol.*, 1907, 63, 459-461.

[5] Holmes, S. J., 'The Instinct of Feigning Death,' *Pop. Sci. Monthly*, 1908, 72, 179-185.

[1] Buttel-Reepen, *Are Bees Reflex Machines?* Medina, Ohio, 1907. (This is a translation by Mary Geisler of a paper which appeared in 1900.)

known that ants possess special organs for producing sound, and Wheeler has maintained that ants communicate by means of sounds; yet, since the researches of Huber, Forel and Lubbock yielded negative results, ants were considered deaf. Weld performed certain experiments which indicated that certain ants hear; but Fielde and Parker's insistence that ants do not respond to aerial vibrations discredited his results. Researches[2] published during the past year demonstrate that ants hear. When outside the nest they pay little or no attention to sounds; but when inside they respond to them with very active movements. This is true even when precaution is taken to prevent the sound waves reaching them through any medium other than the air.

II. Vision.

A critical experimental study of the color vision of the higher invertebrates is much needed. What has been accomplished, during the past year, for ants confirms former statements that they pay little or no attention to the color of the pathway,[2] and shows that their reaction to colored lights is a true visual response.[3] Buttel-Reepen[1] states that bees possess color vision and bases his belief on the following observations:

1. Some bees, on a dark day, mistook a gable that had been painted sky-blue for the open air and tried to fly through it.

2. "A weak after-swarm, mostly of young bees from a hive painted blue, dispersed among the masses of humming bees which were just taking their flight of orientation out of the other hives (which, as is usually the case in Germany, Switzerland and Austria, were standing close together), and settled here and there in clumps. After a short time they flew back to the bee-house, but only a few found the right hive; the rest flew to other colonies, and to which? Only where a blue color invited them did they attempt an entrance and nowhere else." This shows a discriminating vision, but not necessarily the ability to distinguish hues; for in each of these cases the blue may have appeared to the bees as a grey.

III. Distant Orientation.

Another disputed point upon which recent studies have thrown some light is, How do ants and bees find their way home? Bethe insisted

[2] Turner, C. H., 'The Homing of Ants,' *Jour. of Comp. Neur. and Psy.*, Vol. XVII., 1907, pp. 367-435, pl. II.-IV.

[3] Pricer, Jno. L., 'Life History of the Carpenter Ant,' *Biol. Bull.*, Vol. XIV., 1908, pp. 177-218.

that bees are guided home by an unknown force which acts reflexly and that ants are similarly guided by a doubly-polarized odor trail. As far as bees are concerned, Buttel-Reepen[1] has shown the weakness of Bethe's experiments and reports that bees on leaving the hive for the first time make a careful examination of the surroundings. Bees that are not permitted to do this cannot find their way home; neither can bees find the way home after dark. All this militates against Bethe's contention. As for ants, the work of both Pricer[3] and the reviewer[2] prove that ants do not slavishly follow the odor trail. By means of a large number of experiments upon several species of ants, it has been demonstrated[2] that ants find their way home neither by a homing instinct nor by an odor reflex, but by *learning* by experience and by utilizing certain reference data, one of which is the direction of the rays of light. What they once learn is remembered for some time. Pricer's[3] work on ants and Buttel-Reepen's[1] on bees give additional evidence that the hymenoptera possess memory.

It has been shown[2] in the following manner that ants have memory of position or knowledge of location. An ant was trained to mount a section lifter and be carried to and from a table top to an elevated stage from which the ant carried pupæ to the nest. In training the ant, the section lifter was always presented at the same place. After the ant had become accustomed to using the section lifter as an elevator, it would always go to the same place on the table and wait for the presentation of the section lifter. Pricer[3] also claims that ants have memory of location.

IV. Observation on the 'Time Sense.'

That bees possess not merely a knowledge of space, but also a knowledge of time, has been suggested by Buttel-Reepen's[1] observation that, when the buckwheat is in bloom, bees, which normally fly at all hours of daylight, fly only during those hours that the buckwheat gives up its nectar; and proven by the following experiment of Forel's:[4] A table was set in the open, upon which breakfast was served between 7:30 A. M. and 10 A. M., dinner at noon and lunch between 4 P. M. and 5 P. M. Jam, of which bees are quite fond, was served for breakfast and lunch, but not at noon. The bees visited the table in great numbers; but after the first few days they came only between the hours of 7 and 8 A. M. and between 4 and 5 P. M. After things had continued thus for some time, the table was set as

[4] Forel, A., 'Mémoire du Temps et Association des Souvenirs chez les Abeilles,' *Comptes rendus d'Association Française pour l'Avancement des Sciences*, 1906, pp. 459–464.

usual, but no jam was placed thereon. Between 7 and 8 A. M. the bees came in great numbers and searched diligently everything thereon, especially those vessels that had formerly contained jam. This search was continued until nearly 10 o'clock. At noon only one bee came, at 4 o'clock many came, but did not tarry long. The next day the same conditions were repeated. A few visited the table, but they did not tarry long.

V. Methods of Communication.

Can bees and ants communicate? For bees Buttel-Reepen[1] says, yes, they communicate by means of sounds. He bases his assertion upon his observations that bees respond to sounds made by other bees and that they always respond to similar sounds in the same way. For ants this has long been an open question. Pricer[3] says they do; but since his assertion is based upon an experiment which does not preclude the possibility of the reaction being merely a response to a strange odor, the problem is still unsolved.

VI. Evidence for Formation of 'Practical Judgments.'

Not only do the hymenoptera have sensations and memory; but, under certain conditions ants seem to form what Hobhouse calls practical judgments. The experiments[5] upon which this conclusion is based may be epitomized as follows: (1) An ant of a colony of *Formica fusca* var. *subsericea* was noticed to construct a partial bridge of three sections upon the ditch that surrounded the Lubbock island upon which it was housed. (2) A colony of *Camponotus herculeano-ligniperdus* had been residing for weeks in a Janet nest, the outer opening of which was quite large. An ant almost always mounted guard in that entrance. After this guard had been repeatedly irritated with dissecting needles, it withdrew from the entrance, which the ants plugged with detritus. (3) A colony of *Formica fusca* var. *subsericea*, which had lived for many generations where it would be impossible for a crack to appear in the top of its brood chamber, was captured and housed in a Janet nest. A crack was made in the top of the brood chamber. A few ants soon covered this with coarse trash. The trash was removed by the experimenter only to be replaced by the ants. After this had been repeated several times the ants, instead of piling coarse trash on top of the crack, closed it from within by building up from the floor a felted wall of fine particles. Control experiments were made.

[5] Turner, C. H., 'Do Ants Form Practical Judgments?' *Biol. Bull.*, Vol. XIII., 1907, pp. 333-343.

Here we have a utilization of instinctive activities, without a period of preliminary experimentation, to meet adequately conditions for which the ants had no stereotyped response. Such behavior on the part of an animal Hobhouse says gives evidence of the ability to form practical judgments.

VII. Social Character of Certain Responses.

A recent French article[6] lays stress upon the idea that a bee colony is a community in which the individual intelligence has been subordinated to the group intelligence. He thinks that among them we have intelligent coöperation and what he calls collective reasoning (raisonnement collectif). His conclusion is based upon experiments like the following: If we tie pieces of comb in the frames of a hive with a string, the bees will cement the pieces together, construct cells in the gaps and then remove the string. In removing the string, certain bees cut it into pieces. Other bees, coöperating, carry the pieces out of the hive, several assisting with each piece. He placed a large lump of sugar in a certain place. Bees discovered it, but could neither carry it away bodily nor bite off small pieces. They returned to the hive and were joined by others. They then obtained water and dropped it on the sugar to form syrup. The syrup was carried to the hive. This experiment was repeated with similar results. Both of these experiments, and others in his article, are most interesting; but, to explain them, we certainly do not have to predicate anything higher than what Hobhouse calls a practical judgment; and if, as I understand him to say, the bees always act this way under similar circumstances, then the above responses are nothing more than remarkable instincts.

In a work[7] abounding in observations and experiments Wheeler publishes the following conclusions which will be of interest to students of animal behavior: (1) In the lives of social insects, the philoprogenitive instincts are of such transcendent importance that all other instincts become merely tributary or ancillary thereto. (2) The social life itself is merely an extension of these instincts to the adult offspring. (3) The philoprogenitive instincts arose and were highly developed among the solitary ancestral insects long before social life made its appearance. (4) The phylogenetic differentiation of caste arose in the sphere of function before it manifested itself in structural peculiarities.

[6] Bonnier, Gaston, 'Le Socialisme chez les Abeilles,' *Bull. de l'Inst. Général Psychologique*, 7 Ann., pp. 397–426.

[7] Wheeler, W. M., 'The Polymorphism of Ants with an Account of Some Singular Abnormalities Due to Parasitism,' *Bull. Amer. Mus. Nat. Hist.*, XXIII. (1907), pp. 1–93, pl. I.–VI.

(5) The foraging instinct may be due to chronic hunger. (6) Regulation takes place in the sphere of instinct as well as in the sphere of morphology. (7) The social instincts cannot be used to support any of the mechanical theories of development.

Bethe's contention that ants and bees are merely reflex machines, to which we must not even ascribe sensations, is no longer tenable; for the researches of the past year have demonstrated that the behavior of the higher invertebrates is not a series of tropisms, but the result of a psychic complex ranging from simple sensations up through memory. Although we have no evidence of reasoning in its highest phases, yet here and there we catch glimpses of what Hobhouse calls a practical judgment.

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RECENT LITERATURE ON MAMMALIAN BEHAVIOR.

1. THE ESTABLISHMENT OF HABITS.

In an important contribution to the behavior of raccoons, Cole[1] describes in detail their methods of learning to release the fastenings of various problem boxes. The boxes were similar in nature and complexity to those used by Thorndike, Kinnaman and others in their tests upon monkeys.

The time records of the four raccoons for the learning of these fastenings show greater variability than those of cats and monkeys. In the rapidity of forming associations, the raccoon stands almost midway between the monkey and the cat. In regard to the complexity of association, Cole states that the raccoon deserves a place closer to the monkey than to the cat. Davis[2] in a similar study on a much larger number of raccoons (12) reaches a conclusion similar to the above. On page 475 of his article he plots a curve (6), the data for which was obtained from Kinnaman's study of the monkey. The curve represents a series of thirty trials each on nine simple locking devices. Davis compares this with a similar curve obtained from the raccoons on the same (or closely similar) devices. The general character of the learning curves for both animals is the same. According to Davis, the monkeys are a little less clever at the start than the raccoons. Also the curve for the monkeys is slightly more irregular than

[1] Cole, L. W., 'Concerning the Intelligence of Raccoons,' *Jr. Comp. Neurol. and Psy.*, Vol. XVII., No. 3, May, 1907.

[2] Davis, H. B., 'The Raccoon; A Study in Animal Intelligence,' *Am. Jr. Psychology*, Vol. XVIII., No. 4, 1907.

that of the raccoon; this would point to the conclusion that the monkey is slightly more open to distraction than the raccoon. Cole and Davis are thus seen to be somewhat at issue on the question of variability.

Davis brings out the added points that the young animals are more reckless in their expenditure of energy than the old, consequently they solve the various problems at first more rapidly than the adults; and that all through their work the young animals show a greater variability than the adults.

An interesting contested point has been raised between Davis and Cole: Cole found that when he changed the position of fastenings the trained animals never began work at the parts of the box where the fastening had previously been placed, but went at once to the new position of the fastening. Davis' results are opposite: He found that several of his animals did persistently work at the place where a mechanism customarily had been located before finally discovering it in its new position. The question is raised here because the reviewer has found, as has every other investigator up to the present time, that the 'position' error is the most fundamental and deeply rooted error which occurs in mammalian reactions. If Cole is right, it would appear that the sensory control of the raccoon is effected more largely by means of distance sense data than that of most mammals which have so far been observed in the laboratory. Davis suggests that Cole's results in this particular are exceptional.

Yerkes[3] in his study of the dancing mouse presents results on the learning of simple mazes by those animals. He makes the astonishing statement that the dancer did not form a perfect habit in labyrinth *A* (only fairly complex). He states that the fault is not in the nature of the maze but lies in the fact that no motive for following the correct path could be made sufficiently strong (the dancer does not withstand the effect of hunger according to Yerkes). The common mice tested in this same maze learned rapidly to escape from it. In mazes of other types, by using punishment (electric shocks) as a stimulus to escape, he succeeded in establishing uniform maze habits in the dancer.

II. Tests on Vision.

Yerkes[3] presents a satisfactory account of the reactions of the dancer to chromatic and achromatic light stimulation.

He finds that the dancer can discriminate white from black perfectly and that two grays lying as near together as Nendel's Nos. 10 and 20

[3] Yerkes, Robert M., *The Dancing Mouse*, The Macmillan Co., New York, 1907.

can just be discriminated. In regard to Weber's law, Yerkes states that a difference of one tenth is sufficient to enable the dancer to distinguish two lights in the case of three standard values 5, 20 and 80 hefners.[4]

Cole tested the ability of raccoons to discriminate black from white, black from hues, and between complementary colors. In his first test the animals in order to obtain their food had to select a box covered with black, white or colored paper. Since the employing of colored papers in tests on color vision is embarrassed by our inability to control the factor of brightness, Cole's results may be classed possibly as discriminations between grays. His black-white test is the only one where the conditions are unambiguous. In the first experiments the animals had to select the black box from the white: He found that they learn to discriminate black from white in from 70–90 trials. His later tests were made with an apparatus devised by himself and called a 'card displayer.' Here the different grays and colored cards were shown in succession and not simultaneously as has usually been done. He was enabled by this method to get the animals to distinguish between black-white, black-red, black-yellow, black-blue, black-green, blue-yellow and red-green. The complementary color (?) discriminations took longer than either the black-white discrimination or the black-color discrimination. He makes the important statement that the range of effective visual discrimination is extremely short, never more than 18 inches. Davis' results on visual discrimination in raccoons confirm those of Cole, except that Davis, since he presented six possibilities of choice instead of two, obtained a relatively smaller percentage of right choices.

Yerkes,[3] working with approximately monochromatic light filters on the dancer, is the first investigator to arrange an adequate test for color discrimination. His own conclusions are stated as follows: "Although the dancer does not possess a color sense like ours, it probably discriminates the colors of the red end of the spectrum from those of other regions by difference in the stimulating value of light of different wave-lengths, that such specific stimulating value is radically different in nature from the value of different wave-lengths for the human eye, and that the red of the spectrum has a very low stimulating value for the dancer." This conclusion, if correct, concerning the difference in the

[4] In my review of Yerkes' book in the *Jour. of Philosophy, Psychol. and Scientific Methods*, I misstated Yerkes, by saying that a satisfactory demonstration of Weber's law could not be obtained in the case of the brightness vision of the dancer.

stimulating effect of waves of different length, is a fundamental one in our further consideration of tests on color vision. If it be still further substantiated, it clearly shows the uselessness of testing the color vision of animals with light reflected from colored papers.

O. Pfungst[5] shows that nearly all of the wonderful answers returned by the horse, *kluger Hans*, were reactions to visual cues, such cues being afforded by the involuntary movements made by his questioners. If Hans could see his questioner, he could answer any question which could be answered by tapping a certain number of times witl the foot. The way in which Hans reacted to the involuntary move ments of his questioner may be seen in the following extract from Pfungst's book: After Herr von Osten (the owner of Hans) had stated the problem, *he tended always, involuntarily, to bend the head and trunk slightly forward*, whereupon Hans would extend his right foot and begin to tap without putting his foot back after each successive tap. When the desired number of taps was reached, Herr von Osten *would, involuntarily, give a slight upward jerk of the head.* At this second signal, the horse would retract the foot to its normal position. By this study of Pfungst we are shown the delicacy of the visual cues to which the horse can react, the width of his range of vision and the delicate functioning power of his peripheral retina.

Cole[1] has shown that raccoons learn easily to discriminate between square cards and round cards, and between two square cards, 6.5 × 6.5 inches and 4.25 × 4.25 inches respectively. The animals, owing to their previous practice in brightness discrimination, learned to make the correct responses very rapidly.

Yerkes[3] tested the dancer's ability to discriminate between a round food box (5 cm. in diameter and 1.5 cm. in depth) and a rectangular box (8.5 cm. in length and 2.5 cm. in depth). He found that the dancer, under the conditions of the test (direct hunger-food stimulus, without punishment), could not make the discrimination. Yerkes urges, in commenting upon this test, that the negative results obtained do not preclude the possibility that the animals might have formed the discrimination provided a sufficiently strong motive were at hand. However, in a test of discrimination between an illuminated circle and a form of cross, in which he used the punishment method, he failed to get the desired reaction.

III. Auditory Discrimination.

Cole[1] showed that his raccoons learned to discriminate between the highest tone, A_1, of a French harp, and its lowest tone, A″.

[5] Pfungst, O., *Kluger Hans, das Pferd des Herrn von Osten*, Berlin, 1907.

IV. Tests on Retention.

Cole[1] found that the retention of simple fastenings was perfect after intervals of three or four days, and in some cases even after two weeks. His tests with a box possessing very complicated fastenings (7 separate acts), which proved to be about the most difficult box the raccoons could learn, are interesting: Raccoons Nos. 3, 2 and 1 were tried on this box after an interval of 147 days. Only No. 3 showed a perfect mastery after such an interval. The other two worked almost, but not all, of the fastenings. Cole considers this interval to be about the maximum for the retention of complicated fastenings. Davis likewise shows that simple fastenings are retained by the raccoon for periods of one year. In a crucial test he found that the association of a complicated fastening persisted, with a certain loss, over a period of 286 days (including hibernation). An expression of the efficacy of retention in that instance was determined as follows: On relearning after this period, 24 trials were necessary to attain the perfect standard which had in the original learning process required 107, and the times of these 24 trials were much shorter than the first 24 trials of the primary learning process.

Yerkes[3] shows that the dancer's retention of a black-white or white-black discrimination may persist over an interval of from two to eight weeks. Retention of such a habit, however, is seldom perfect after four weeks. The same author shows that a habit (black-white discrimination) which after an interval of eight weeks has been lost by the dancer can be reacquired in a much shorter time than was necessary for the original acquiring of it. He gives indices of modifiability as follows:

	Learning.	Relearning.
Females,	104	42.5
Males,	72	54

Yerkes mentions the fact that there are great individual differences in the retention of discrimination habits. In this connection he shows that the acquisition of one labyrinth habit aids the animal in learning a different labyrinth.

V. Learning from Being Put Through an Act.

A large part of Cole's[1] paper is devoted to proving that, contrary to Thorndike's results on cats, raccoons will learn to reënter a box of their own accord into which they have been passively dropped and then allowed to go out and get the food; and further that they can learn fairly complicated sets of acts by being put through them. Cole

generalizes on his first set of experiments as follows: "Since four raccoons exhibited this reaction, it is safe to conclude that any raccoon which has been lifted into a box and allowed to come out and be fed will sooner or later go in of his own accord, and further that he will go in before the 100th trial and probably before the 75th trial as my four animals did. The behavior of these animals forces one to believe that it dawns on the animal that he can hurry the matter of getting food by rushing back into the box and coming out again. . . . I should say, rather, that it had an image of the interior of the box as the starting point of the food getting process and an idea of going back to recommence the process."

In order to test whether the animals could learn to undo fastenings by this artificial method of teaching the following experiments were tried: Two raccoons were allowed to learn certain problem boxes by trial and error, and two (the stupider two) were put through the acts. The average time of the first success in manipulating each of the 11 boxes was, for the animals which were put through the act, 41.6 sec.; for those not put through, 90.2, or more than twice the above average. In these tests, the animals did not always *perform the act in the way in which it had been taught.* They were put through the acts with one forepaw: they performed the acts with that paw, with the other forepaw and with both forepaws. "*And exactly the same is true of those who learned the fastenings by trial and error*" (italics mine).

The animal can be made to learn the act in the way in which he was put through it. "If the act which he is put through is the one which will remain the easiest and most convenient for him throughout the tests, irrespective of his position in the box, he will never vary from it. If not, he will employ your act when his position makes it convenient and he is looking at the latch you began with." (Surely he does not mean this statement to be taken literally!) Cole further says that the raccoon can learn and act from being put through it, even though it has failed to learn it by its own efforts.

In commenting upon Cole's results, it may be said that in his discussion he has left out one or two very important factors: First and foremost, putting the animal through a large number of times would naturally lead it to attack the *moving parts* of the apparatus when trying to solve the problem alone. In other words, putting the animal through need not tend to produce the rise of an idea; it might have the effect merely of limiting the area of attack upon the problem-box. The learning process as he describes it even when the animal is put

through was slow enough for us still to be able to call the type of learning involved merely 'abbreviated trial and error.' The fact that the animals put through did not use the same system of movements employed in putting them through the act, but one similar to those utilized by the animals which had learned by trial and error, leads one to think that what was gained by the animal was the ability to attack the problem-box at the point where the mechanism is located. Then, too, the situation is not new to those animals which have been 'put through' the problem, and the diffuse, time-consuming, motor overflows usually present on all first trials are not present in their case.

VI. Learning by Imitation.

Cole[1] maintains all through his paper that he has found abundant evidence of imagery and of the rise of ideas in his raccoons, yet he was unable to find the function of imitation present. "I have no evidence that the raccoon imitates his fellows. Long attention to the experimenter's movements apparently arouses in the animal an impulse to attempt the act itself, but this impulse may be entirely spontaneous." Davis[2] notes only one or two cases of possible 'instinctive' imitation, but claims that he observed no cases of imitation of a higher order of one animal by another. Yerkes[3] after varied and complete tests on the dancer finds no evidence of imitation: "Although abundant opportunity for imitation in connection with the opening of the doors in the discrimination box was given to twenty-five, I obtained no evidence of ability to learn by imitation." The same author's experiments upon the imitation of a climbing feat are just as conclusive in showing the absence of any tendency to imitate on the part of the dancer.

Watson's[6] experiments on rhesus and cebus monkeys show (in the particular animals studied at least) that the function of imitation in its higher forms is lacking. Some evidence for a circular type of reaction was offered.

Berry,[7] on the other hand, in a paper on the imitative tendency of the white rat finds evidence both for an instinctive and a possible voluntary type of imitation. His method of studying imitation was to allow one animal to establish an association (problem box of the manipulation type) by the trial and error plan and then to allow a second untrained animal to be present with the trained animal as the latter per-

[6] Watson, John B., *Imitation in Monkeys*. See this number of the BULLETIN.

[7] Berry, Charles Scott, 'The Imitative Tendency of White Rats, *Jour. Comp. Neurology and Psychology*, Vol. XVI., No. 5, September, 1906.

formed the necessary act. The untrained animal was then tested alone for a certain number of minutes. If he failed in the allotted time, the trained animal again showed the trick to the untrained animal. Berry lays emphasis upon the watching of one rat by another and assumes that the imitation (?) occurred through a visual process. He made no effort to test the visual acuity of the white rat, but apparently assumes that it dwells in a highly organized visual perceptual world[8] (which would be a *conditio sine qua non* in any act of imitation which was visually initiated). The work of Small, of Watson, and of Carr and Watson has established (if any fact in comparative psychology can be said to be established) that the rat uses kinæsthetic data for control in all situations like that of the maze; Dr. Florence Richardson (whose results are at present unpublished) has just shown that vision is practically a negligible factor in the reactions of the rat to problem boxes of the type used by Berry and that the sensory control in these situations as in those offered by the maze is kinæsthetic. In the reviewer's mind, this mass of data makes Berry's results entirely untenable so far as showing the presence of imitation in the white rat. What factors may have been present to give Berry such results are at present not known to the reviewer. Several possible ones were certainly present: Individual variation; more thorough habituation to box on part of imitatee; determination of specific stimulus (*i. e.*, position of mechanism, going out of door, etc.); but possibly more important still is the subtle way in which the presence of one animal may influence another by raising the general physiological tonus of a second animal. As an example of this I cite the following: Four female rats were especially lazy and stupid in learning the maze; each would enter, work for a few moments and then lie down. If now another animal was put in the maze, the former would immediately show signs of activity and begin again upon the problem. This increase in the general tonus of the organism as a whole was in all probability brought about partly through contact stimulation, partly through general olfactory stimulation, and through specific olfactory stimulation by the odor of the sexual organs.

The same author's[9] experiments upon imitation in Manx cats are almost equally open to criticism — it is still an assumption (here possibly a safe one) even in the case of Manx cats to proceed upon

[8] Cf. Mead, G. H., 'Concerning Animal Perception,' PSYCHOLOGICAL REVIEW, October, 1907.

[9] Berry, Charles Scott, 'An Experimental Study of Imitation in Cats,' *Jour. Comp. Neurology and Psychology*, Vol. XVIII., No. 1, 1908.

the supposition that *vision of a kind suitable to the perception of the acts of another animal* is present. His experiments were conducted upon an adult Manx female and her three kittens. The type of problems resemble in some cases those used by Hobhouse, such as getting food (with paw) from a bottle, rolling a ball into a hole, climbing down from a table, learning to catch and kill mice, etc. In all of these experiments, the author finds evidence of the presence of imitation. On the whole, the work on the cat demands slightly more scientific consideration than that on the rat, but the reviewer is far from accepting the conclusions of Berry — especially the conclusion he reaches in regard to the catching and killing of mice. To draw the conclusion that the young cat catches, kills and eats mice by imitation from such uncontrolled, experimental evidence as Berry offers — evidence obtained from only one litter of cats and from a very short part of the life history of the animals at that — comes fairly close to making inferences without evidence.

VII. Experimental Evidence for the Presence of Mental Imagery.

Cole[1] cites in detail an interesting reaction on the part of his raccoons which he believes cannot be explained, even taking into account the law of parsimony, without maintaining the presence of visual imagery. The reaction referred to appeared in tests made upon the 'card displayer' mentioned above. Two cards, say a red and a green, were presented in succession; the animal had to react to the green card by climbing up on a high box for food, but had to remain inactive when the red card was displayed. After the animals had been tested in this way for some time, they began clawing the 'no food' card (red) down, and sometimes the 'food' card (green) up. This variation in response was fostered until some of the animals became fairly proficient in the act. Cole reasons concerning it as follows: "When the animal thus reacts perfectly to red and green, and in addition busies himself in clawing the red card down and the green card up, surely his discrimination of the two is perfect. Now we are forced to ask, *Why should he put the red card down if it did not fail to correspond with some image he had in mind*, and why, when he put the green up, should he leave it up and go up on the high box for food if *the green did not correspond with some image he had in mind?*"

The responses thus obtained are exceedingly interesting, but it certainly strains our credulity to suppose that these animals can have

separate images for a series of different shades of gray. (Cole in a later place in the paper admits that the animals so far as visual discrimination is concerned were probably reacting to the differences in the white values of the cards.) Davis[9] attempts with some success to offer a different explanation for the above reaction. He suggests that "*It was an accidental result of the raccoons' inveterate impulse to attack and manipulate anything that can be moved.* The animals had already associated the colored cards with the getting of food; had earlier still been accustomed to get food by attacking some sort of fastening; from this it is a short step — hardly a step at all — to attack card-holders. After having succeeded a few times in thus starting the train which leads to feeding, the activity would become stereotyped like the opening of boxes or any other." The reviewer fails to see why this explanation of Davis' is not an adequate one; it is certainly more nearly in line with what we know elsewhere of mammalian activity.

The incidental observations running all through Cole's work, lending support to his contention regarding the presence of imagery, are certainly not of a convincing character.

VIII. The Kinæsthetic Character of Certain Sensory Control Processes.

Carr and Watson,[10] in experiments with a new form of maze, have found that the white rat trained to run the maze always from the entrance, attains orientation, when put down at unfamiliar starting points in the maze, by making certain exploring movements before getting the cue which leads to the establishment of the automatic character of the remaining part of the series of acts. By detailed experiments which cannot be cited in the review, the authors concluded that, in obtaining orientation, a *kinæsthetic cue* might serve the same purpose for the rat as a distance sense cue for man.

In this same paper the authors advance still further evidence for the general kinæsthetic character of the sensory control of these animals. This evidence came from tests on a maze, the straightaways or alleys of which could be lengthened or shortened at will without disturbing the number of the turns or their relations. The animals which had learned the lengthened form of the maze and were then suddenly introduced to the shortened form, in nearly all cases ran squarely into the ends of the alleys affected by the change. On the average, sixteen trials

[10] Carr and Watson, 'Orientation in the White Rat,' *Jour. Comp. Neurology and Psychology*, Vol. XVIII., No. 1, 1908.

per rat were necessary to restore automatic adjustment to the changed conditions. On the other hand, the animals which had become habituated to the shortened form and were then introduced to it in its lengthened form attempted to round corners at the old distances regardless of the fact that the alley into which they then should have turned was further along in the course.

Yerkes[3] in testing the dancer in the labyrinth states that, after the maze is learned, probably no sense data is necessary for the guidance of these animals in the performance of such a series of acts: "A habit once formed, the senses have done their part; henceforth it is a motor process, whose initiation is conditioned by the activity of a receptive organ (at times a sense receptor), but whose form is not necessarily dependent upon immediate impressions from eye, nose, vibrissæ, or even from internal receptors. These are statements of my opinions; whether they express the truth, either wholly or in part, only further experimentation can decide." The reviewer does not know clearly what these statements mean. If Yerkes means to say that a series of acts learned in the beginning (consciously or unconsciously) by means of the activity of the distance sense receptors or of internal receptors or by means of both combined, can be later carried out by means of kinæsthetic-motor responses alone (without the accompaniment of consciousness), he would be restating merely the generally accepted teaching on habit. If, on the contrary, he means to imply that there is even in a perfectly established habit a cessation of neural functions of the internal (kinæsthetic) receptors, every case of locomotor ataxia or other sensory disturbance should lead him to change such an opinion.

It is to be regretted that neither Cole[1] nor Davis[2] in their respective studies on the raccoon devised tests for isolating the function of kinæsthetic sensations (receptions).

J. B. W.

THE ANIMAL MIND.

The Animal Mind. A Text Book of Comparative Psychology. MARGARET FLOY WASHBURN. New York, Macmillan, 1908. Pp. x + 333. $1.60.

Professor Washburn's book is the second of a series of volumes on animal behavior, of which the first appeared in 1907 entitled *The Dancing Mouse* by Dr. Robert M. Yerkes, the editor of the series.

In general design and arrangement of matter the book is adapted for the purpose of a text-book of animal psychology.

The author has rendered a distinct service to animal psychology in having gathered together in compact and presentable form the results of numerous experimental researches in the field of comparative physiology of the senses and of comparative psychology.

The book is admirably planned, exhibiting a careful grading of the subject-matter in chapters, whose sequence is seen immediately to be the most logical one.

Chapter I. discusses the difficulties in the way of the comparative psychologist and the methods of obtaining and interpreting the facts. The author here gives in a few pages a brief history of this, one of the youngest of the sciences and classifies into three groups the investigators contributing to it, according to the degree in which they are willing to admit the existence of consciousness in the interpretation of animal behavior.

After a brief chapter on the evidence of mind in animals, and one on the mind of the simplest animals which presents the results of Jennings' researches on the protozoa, the author takes up the subject of sensations.

Since the ability to discriminate between stimuli is a subject which lends itself most readily to experimental investigation, four chapters are devoted to this subject, each case of discrimination between stimuli being held to be a discrimination of differences in sensation *provided consciousness exists in the animal in question*. The methods of investigating discrimination in so far as they involve observation of behavior may be given as: The method of preference; method of extirpation of a sense organ; use of localized stimuli; the independent fatiguing of reaction; difference in reaction time; and lastly, combinations of these methods. Miss Washburn states that the preference method is unsatisfactory in that no preference may exist where discrimination is possible. This is obviously true, but the value of the preference method as used by Graber need not be underestimated so long as the existence of preference gives a positive proof of discrimination. The author states that the first requisite is to give the animal a motive for its choice. To go through the process of giving the animal a motive where one already exists is superfluous, and certainly the preference method is merely the taking advantage of an already existing motive. Those who have done any extended work in the field realize that this very giving of a motive is the most tedious and time-consuming part of the experiment and is to be resorted to only when no natural motive is present. Before the preference method is denounced wholesale, it must be observed that the giving of

a motive for choice is itself a method of preference. We would wish that this method might have been added to the list, giving a method of *natural preference* and a method of *taught preference.* Both should be retained, the former being most valuable as a time saver and the latter to be used in default of the applicability of the former.

The names given the other methods are not on an equal basis, some referring to the way in which conditions are applied, while others have reference to what is expected of the animal.

The results of experiments on sensory discrimination are grouped in this chapter under the heads: The chemical sense, hearing and vision, each chapter treating of these senses in phylogenetic order.

Two chapters, VIII. and IX., consider the topic of space perception, and take up in order: (I.) Reaction to a single localized stimulus; (II.) orienting reactions; (III.) reaction to a moving stimulus; (IV.) reaction to an image; (V.) reaction to distance.

Chapters X. and XI. discuss the modification of conscious processes by individual experience. The way in which useless movements are dropped off is taken up in a review of those experiments involving the use of the labyrinth and the puzzle-box. Another form of modification, one in which the dropping off of useless movements does not seem to be the most prominent feature, is given as the learning to inhibit instinctive action, either with or without choice. The methods of studying this are in general those now used in studying sensory discrimination. The discrimination may be between stimuli presented successively or simultaneously. Pain, in Miss Washburn's opinion, is a stronger modifying force than pleasure.

The author is inclined to doubt the importance attached by some experimenters to the rôle of kinæsthetic sensations in the learning of animals. Watson concludes from experiments on the rat in the Hampton Court maze that the guiding factor is kinæsthetic, on the ground that rats, after being deprived of all the special peripheral sense organs, could run the maze successfully. Miss Washburn criticizes this conclusion in the present book as well as in a subsequent review of work with rats, suggesting that "a habit may be quite independent of the stimuli that served to form it, as the pianist becomes independent of the notes in playing a familiar piece"; that is to say, the guidance is *turned over* to the kinæsthetic sense. Kinæsthetic sensations may act as a guide in movement when other senses have aided in learning the movement. We would expect, however, that the blinded, anæsthetized animal would take longer in performing the required act, since the transfer of an activity from under the domain

of one sense to that of another always involves new coördinations, but the rats in Watson's experiments after being blinded lost no time in running the maze as compared with the time they consumed in running it before the operation; and furthermore, a fact which Miss Washburn seems to have missed altogether, the blind and the anosmic rats *learned* the maze just as quickly as the normal rats; in fact their time was a little shorter. Just *how* the kinæsthetic sensations suffice in the learning process is a question that comparative psychologists have not answered satisfactorily. It seems clear that their rôle in the human being at least is mainly that of guiding actions which have been learned through the higher senses.

In man, after an act is learned and the control is taken over by the kinæsthetic sensations, consciousness is free to be applied to other things. Now just what may be supposed to occupy the animal's attention during the learning and after the act has been learned? The writer's position on this point is not made clear. We find the statement: "As the learning process proceeds, objects come to stand in the focus of attention, so that to the cat in the puzzle box, the string that opens the door is immediately attended to. The monkey becomes aware of the difference in color between vessels otherwise quite similar." The opposite view is equally tenable, *i. e.*, that as the learning process proceeds, the objects are *less* attended to; the string, etc., setting up an immediate motor response (unconscious) while the animal's consciousness either lapses or is concerned with some other object, *e. g.*, the food. The reviewer has elsewhere suggested the idea that in such cases it is the desire for the reward, suffused with a pleasurable or painful feeling-tone which fills consciousness.

In a chapter on the memory idea the theory is propounded that memory depends upon a delay between an incoming stimulus and the reaction, so that the nervous energy may have time to impress the sensory centers. This is correlated with the view that the distance receptors — to use Sherrington's term — are most fruitful in yielding memory images since their reactions are not so immediate. That memory images depend upon associations and that the formation of associations takes more time than immediate responses seems to be the accepted way of saying the same thing. We must take exception to the statement that the distance receptors do not bring about immediate reactions. Sounds are notable for the immediacy with which they occasion motion. Again, some of the most vivid memories are of experiences which have been most momentary.

In a final chapter the writer emphasizes the factor of attention in securing prepotency of certain ideas over others.

The bibliography of 29 pages is indeed a delight to the animal psychologist. The works cited represent the best of the experimental researches in the field and would constitute an ideal library for the student of animal behavior.

We are glad to have at last a systematic text-book in comparative psychology based upon the results of actual experiments upon animals. Too often do we find that people whose interest in the question of mind in animals has led them to do more or less reading in this field, have started in at the wrong end and have unwittingly made many unwarranted assumptions as to the kind and degree of consciousness which may be supposed to exist in the lower forms.

Although the writer's indebtedness to other comparative psychologists (Yerkes, Jennings and others) is apparent throughout the book, there are several original contributions to the study, these being mostly in the way of method. The strong feature of the work is the systematization and correlation of facts. Perhaps it is most fitting that the writer of the text-book should be one not biassed by being a large contributor to the subject-matter, but one who with perfect fairmindedness and impartiality can present the results of others. This Miss Washburn has certainly done.

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REPORTS.

MEETING OF EXPERIMENTAL PSYCHOLOGISTS.

On April 15 and 16 the fifth annual meeting of experimental psychologists was held in Emerson Hall, Harvard University. Twenty-two psychologists representing fifteen of the eastern laboratories were present to join in the general discussion of experimental problems and to hear informal reports of researches now going forward or recently completed.

The first session was mainly devoted to subjects in the field of vision. Professor Pierce, of Smith College, described the present state of the controversy regarding the checkerboard illusion, demonstrating that the attempts at an explanation in terms of the Zöllner illusion are inadequate and that the objections do not hold which have been raised to the original explanation by means of irradiation. A discussion of the cause of the 'toy-effect' in stereoscopic vision followed, after which Professor Lough, of New York University, presented the results of some mental tests by means of a comparison of lengths; the extent of variation of judgments and the percentage of errors serve as indications of mental ability, fatigue, etc. Professor

Lough also suggested a simple method of determining the point of ocular fixation, by finding the edges of the blind spot. Professor Dodge, of Wesleyan, then described an investigation of eye-movements which, in connection with Dr. Diefendorf, he is carrying out upon insane patients. The ability of the eye to catch and follow a swinging pendulum is determined by the photographic process which Professor Dodge has made familiar. Of the cases of manic depression, general paralysis and dementia præcox which have been examined, the first mentioned group are best able to follow the pendulum, whereas the dementia præcox patients are quite unable to establish the pursuit movement.

At the afternoon session, Dr. F. M. Urban discussed the formal aspects of a psychophysical investigation now in progress at the University of Pennsylvania, the aim of which is to make an empirical comparison of the psychometric functions obtained by the methods of minimal differences and of constant stimuli. This was followed by a general discussion of the place of experimental demonstration in the elementary psychology course. Professor Warren described the procedure in vogue at Princeton, after which different members described various methods and demonstration devices which have been found of value.

Much of the second day was given over to inspection of the Harvard laboratory, with demonstration of the researches now running. In animal psychology, Dr. Yerkes reported a continuation of his studies of the behavior of the dancing mouse, the problems of modifiability and the relation between rate of learning and strength of punishment-stimulus being foremost. Imitation in monkeys, and the transfer of effects of practice in sensory discriminations of chicks, are the subjects of two other investigations. Preparations are being made to study the color vision of mice and frogs. In human psychology, one set of investigations centers about the study of individual differences. Mr. Frost has made correlations of the individual differences of a large number of mental functions in ten observers. Other students are occupied with individual differences of suggestibility, of the scope of attention, and of the rhythmical processes, mental and psychophysiological. Another group of investigations is concerned with the subconscious factors of mental process. How far is it possible to voluntarily inhibit a group of associated ideas from arising in consciousness? How far is the memory for pleasant and unpleasant words different? Still other investigations have for their subjects the æsthetic division of the straight line in vertical, horizontal and diagonal posi-

tions; the motor effects of melodies; the effects of intellectual and emotional activity upon the electrical resistance of the skin, and upon the bodily temperature; and color vision around the blind spot. Professor Holt reported that a study of the mental content present when one thinks abstract and concrete terms leads him to take issue with Ziehen's assertion that there is more in mind for an abstract term than for a concrete.

In the Cornell laboratory, one set of investigations has been directed toward a study of the subconscious, with a view to reducing the limits of the term. Professor Titchener reported that Mr. Pyle and he, working on the after-images from imperceptibly colored discs, have obtained uniformly negative results: when color is seen in the stimulus there is color in the after-image, and not otherwise. Professor Bentley, continuing his experiments correlating clearness and intensity, finds that in sounds an alteration of clearness brings with it an alteration of intensity, and *vice versa.* One group of students is continuing the search for a measure of degrees of attention. Another has taken the cognition times with four pairs of stimuli—wet-dry, hot-cold, hard-soft, and sharp-blunt—and then has taken the pleasant-unpleasant reaction times to these same stimuli: the latter fall about midway between the shortest and the longest of the cognition times. The main work of the laboratory for the year centers about the study of imagination. This is pioneer work, said Professor Titchener, for the distinctive features of the imaginative consciousness, if such there be, have never been clearly determined. One way of approach is to induce cases of memory, imagination, anticipation and so on, and see from the observer's description whether any differentia can be discovered. One investigation seeks to find whether there are characteristics of an image which make it usable as an imagination image but not as a memory image, and *vice versa.* Another establishes a remarkable correlation between acts of memory and eye-movement: 70 per cent. of the instances which were unquestionably memory results give eye-movement, while 80 per cent. of the instances which were obviously imaginations give no eye-movement.

Professor Sanford reported that at Clark University a study of the process of learning type-writing showed that many of the steps of improvement come unintentionally, after which the improvements are consciously adopted. A continuation of the study of the Wheatstone stereoscope with exposure alternately to the two eyes seems to show that there is no stereoscopic effect unless the exposure periods overlap; the depth seems to be dependent upon the amount of overlapping. Other investigations are under weigh as to the mental content in volun-

tary movement; the process of the comprehension of the meaning of phrases; the effect upon the peculiar associative power of odors produced by increasing the complication of the associations; and the time required for æsthetic judgments. Dr. Porter reported investigations in progress on the intelligence of the porcupine; on vocal imitation in parrots and song birds; on the color-vision of ring-neck doves; and on the behavior of spiders. Dr. Porter finds that many of the modifications of behavior in spiders, as in weaving webs of different patterns, are instinctive rather than intelligent adaptations.

Professor Angier and Dr. Cameron were present to speak for the Yale laboratory. Dr. Cameron is making a kinetoscopic study of eye-movements in reading, these being correlated with the spoken word. Up and down eye-movements, as well as horizontal, are found. Dr. Freeman is continuing his studies of writing reactions. In the laboratory at Brown University, Professor Delabarre has in progress three researches, one upon the feelings of pressure in the ear when all sound stimuli are absent, a second upon the effect of the direction of lines in space perception, and a third upon the influence of various factors on energy. By means of tests of a clinical nature which can be used rapidly, daily fluctuations of energy, the influence of amount of sleep, diet, stimulation, etc., are being determined preparatory to a renewed study of the effects of *cannabis indica*. Professor Thorndike mentioned three investigations going forward at Teachers College, Columbia University. One treats of the relationship between the vividness and fidelity of images from one sense and that of images from the other senses, and, contrary to common opinion, finds a very high correlation. There is an 'imaginative' type. Mental measurements of twenty Salvation Army refugees and twenty university people form the basis for a general study of correlations. A third investigation seeks to establish whether the inheritance of intellectual traits is Mendelian or blended.

Full reports from some of the laboratories were not presented because of the pressure of time. As several of those in attendance were compelled to leave on the afternoon of the second day the meetings were brought to a close at that time, but a number remained for another day of visiting and inspection.

A most enjoyable feature of the meeting was an informal reception on Wednesday evening at the home of Professor Münsterberg.

Professor Warren extended an invitation to the psychologists to hold their sessions next year at Princeton.

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